cemented into a more compact rock. I carefully watched the habits of the two species most numerous on the "flats," and in no case did I observe a single individual browsing on the patches of living coral. In truth it was on the dead coral rock forming the "flats" of these reefs that these two species of Holothuriæ subsisted ; and it appeared to me that they selected those feeding-grounds where the attachment of molluscs, zoophytes, and stony algæ had to some degree loosened the surface of the rock.

The particular species, on which my observations were made to determine the amount of coral sand daily discharged, possessed a bluish-black body, from 12 to 15 inches in length when undisturbed, and with a circle of 20 pelate tentacles around the mouth. Without going into all the details of my methods of investigation, it will be sufficient to state that from three independent observations on this species of Holothuria I have placed the amount of coral sand daily voided by each individual at not less than two-fifths of a pound (avoirdupois). At this rate some fifteen or sixteen of these animals would discharge a ton of sand from their intestinal canals in the course of a year, which represents about 18 cubic feet of the coral rock forming the "flat" on which these creatures live. In order to illustrate this point more clearly, I will assume that every rood of the surface of the "flat" supports some fifteen or sixteen Holothuriæ, a number which errs rather on the side of deficiency than of excess. In the course of a year 18 cubic feet of coral rock will be removed in the form of sand from the surface of each rood, which is equal to the removal of 1-605th of a foot per annum, or 1 foot in about 600 years.

Although this estimate can be only regarded as of a tentative character and as applicable to but one species of the Holothuriæ, it nevertheless throws some light on what I may term the "organic denudation" of coral reefs, and it is not unreasonable to suppose that where a fringing reef is undergoing a very gradual up-heaval, the combined operation of the fish, the mollusc, the annelid, and the echinoderm, may prevent it from ever attaining an elevation above the level of the sea at high water.

H. B. Guppy

H.M.S. *Lark*, St. Christoval, Solomon Islands, June 30

Railway Geology—a Hint

It must often have occurred to others as well as to myself when making a long journey by rail, and being whirled along all too fast through section after section of the greatest interest to the eye that can see in them something more than mere railway "cuttings," how valuable would be some handbook giving the geological features of the country traversed by the principal railway lines, and illustrated by clearly drawn maps and sections.

To give an instance—I have occasion pretty often to travel by the South Western line from Waterloo Station to Exeter, a route along which my untrained eye can take note of a succession of instructive pictures, in the course of a five hours' journey—the recent gravels, &c., covered by pine wood in the neighbourhood of Woking, broken abruptly at Basingstoke station by a section of the chalk, to be succeeded from here onwards to Salisbury by undulating downs of the same formation, bare of trees, and but sparsely inhabited ; next, at the Yeovil junction, a sandstone quarry, riddled by martin's nests, presumably of oolitic age ; then, between Axminster and Honiton the greyish blue of a cutting through the lias ; to be finally succeeded, as I approach the term of my journey, by the rich red earths and loams of the new red sandstone.

Any other line, for instance, the Great Western, which runs parallel to that just instanced, would give equally varied pictures ; and a copiously illustrated handbook, with notes explanatory, but as brief as possible—not only of the ground immediately bordering the line of rail, but of the general features of the neighbouring country within the range of the eye of the traveller, should surely, I venture to think, have a large circulation.

Will no geologist—a member of the Government Survey, for instance—undertake the task?

J. C. G.

New University Club, October 27

[We noticed a Guide of this kind for American railways in vol. xix. p. 287, and then suggested the utility of a similar handbook for England.—Ed.]

Complementary Colours

I have often noticed the complementary purple on the foam of the bluish-green waters of Alpine rivers. The waters of the Lake of Geneva, and of the Rhone at Geneva, as is well known, are not bluish-green, but greenish-blue ; but there also I have noticed what to my eye is exactly the same tint of purple on the foam.

Joseph John Murphy

Old Forge, Dunmurry, co. Antrim, October 28

Palæolithic River Gravels

The recent articles and reports in your columns on the subject of Palæolithic river gravels bring three points strongly forward, viz. :—

1. The great number of "flint implements" and "flint flakes" found in the river gravels.
2. The presence in the same deposits of bones of recent and extinct Mammalia.
3. The entire absence of the bones of man.

Such being the uniform results of persevering researches extending now for more than twenty-four years, it is surely time to request anthropologists to give (1) some explanation of the remarkable absence of human remains in deposits containing so many objects considered to be of human manufacture, and (2) some proof that it is absolutely impossible for these so-called "flint implements" and "flint flakes" to have been formed by natural causes.

C. Evans

Hampstead, October 18

LAVOISIER, PRIESTLEY, AND THE DISCOVERY OF OXYGEN

It is a matter of very little importance whether Lavoisier actually obtained oxygen gas a few weeks or days before Priestley. The bare bald discovery of the gas is a very minor matter when placed in juxtaposition with the astounding revolution produced in chemistry by Lavoisier ; with the admirable series of experiments, the acute reasoning, the elegant logical penetration, which enabled him to overthrow the theory of Phlogiston when literally all Europe supported it. The discovery of oxygen dims and pales before the development of the theory of combustion, the theories of acidification, of calcination, of respiration, and the introduction of exact quantitative processes and instruments of precision into chemistry.

But it matters much whether the fair fame of one of the noblest and wisest men in the long roll of illustrious natural philosophers is to remain with a grievous slur cast upon it. It matters much whether his reputation is to be blasted by the reproach that he claimed the discovery of oxygen, knowing well that Priestley had preceded him.

It is with a view of removing this slur upon the memory of the founder of modern chemistry, and certainly not with any thought of adding one iota to his long list of greater triumphs, that we have examined into the true bearings of the question.

First as to the accusations. Dr. Thomas Thomson, in his "History of Chemistry," 2nd edit., 1830, vol. ii. p. 19, writes : "Lavoisier, likewise, laid claim to the discovery of oxygen gas, but his claim is entitled to no attention whatever, as Dr. Priestley informs us that he prepared this gas in M. Lavoisier's house in Paris, and showed him the method of procuring it in the year 1774, which is a considerable time before the date assigned by Lavoisier for his pretended discovery." Again, p. 106 : "Yet in the whole of this paper the name of Dr. Priestley never occurs, nor is the least hint given that he had already obtained oxygen gas by heating red oxide of mercury. So far from it, that it is obviously the intention of the author of the paper to induce his readers to infer that he himself was the discoverer of oxygen gas. For after describing the process by which oxygen gas was obtained by him, he says nothing further remained but to determine its nature, and 'I discovered with *much surprise* that it was not capable of combination with water by agitation,' &c. Now why the expression of surprise in describing phenomena which had been already shown ? And why the omission of all mention of Dr. Priestley's name ? I confess that this seems to me capable of no other explanation than a wish to claim for himself the discovery of oxygen gas, though he knew well that that discovery had been previously made by another."

Had Dr. Thomson been better acquainted with the character of Lavoisier; had he known what manner of man he was in all his dealings with his contemporaries and with the work of those who had gone before, he would never have made such an assertion as the above.

Prof. Huxley in his Birmingham address on Priestley (August 1, 1874) also accuses Lavoisier of unfairness: "though Lavoisier," he writes, "undoubtedly treated Priestley very ill, and pretended to have discovered dephlogisticated air, or oxygen, as he called it, independently, we can almost forgive him, when we reflect how different were the ideas which the great French chemist attached to the body which Priestley discovered."

Starting, as we confess, with the complete belief that Lavoisier did not discover oxygen, we are compelled to assert that a careful perusal of the various memoirs bearing upon the subject and the consistent attitude of Lavoisier throughout, has led us to the firm conviction that he has as much right to be regarded as the discoverer as either Priestley or Scheele.

Let us examine Dr. Thomson's statements. The year 1774 he asserts "is a considerable time before the date assigned by Lavoisier to his pretended discovery."

Lavoisier ("Traité élémentaire de Chimie," 1789, part 1, Chap. III.) says in speaking of oxygen: "Cette air que nous avons découvert presque en même temps, M. Priestley, M. Scheele, et moi, a été nommé, par le premier air déphlogistiqué; par le second, air empyréal. Je lui avais d'abord donné le nom d'*air éminemment respirable;* depuis on y a substitué celui d'*air vital*." Evidently "presque en même temps" is a very loose statement. Scheele's treatise, "Chemische Abhandlungen von der Luft und Feuer," was published in Upsala in 1777, and he certainly did not discover oxygen before 1775. Lavoisier is therefore speaking in quite general terms when he says that oxygen was discovered almost at the same time by Priestley, Scheele, and himself. He at least puts himself on a level with Scheele as to date, and it is universally admitted that Scheele procured the gas after Priestley. And this general expression is the only *claim* to the discovery we can anywhere find in the writings of Lavoisier.

Now what are the facts in favour of Lavoisier? On November 1, 1772, he deposited with the secretary of the Academy a note, which was opened on May 1 following, in which he stated that he had discovered that sulphur and phosphorus, instead of losing weight when burnt, actually gained it, without taking into account the humidity of the atmosphere. He traced this to the fixation of air during the combustion, and surmised that the gain of weight by metals during calcination was due to the same cause. He reduced litharge in close vessels "avec l'appareil de Hales," and observed the disengagement of a great quantity of air. "This note leaves no doubt," says Dr. Thomson, "that Lavoisier had conceived his theory, and confirmed it by experiment, at least as early as November, 1772. . . . "Il est aisé de voir," writes Lavoisier, just before his death, "que j'avais conçu, dès 1772, tout l'ensemble du système que j'ai publié depuis sur le combustion."

Early in 1774 he published experiments in his "Opuscules physiques et chimiques," to prove that lead and tin, when heated in closed vessels, gain weight, and cause a diminution in the volume of air. "J'ai cru pouvoir conclure," he writes, "de ces expériences, qu'une portion de l'air lui-même, ou d'une matière quelconque, contenue dans l'air, et qui y existe dans un état d'élasticité, se combinait avec les metaux pendant leur calcination, et que c'etait à cette cause qu'était due l'augmentation de poids des chaux métalliques." Later in the year he read before the Academy ("à la rentrée publique de la Saint Martin, 1774"); a memoir "On the calcination of tin in closed vessels," in which he proved that when tin was calcined in hermetically sealed vessels, it absorbed a portion of the air equal in weight to that which entered the retort when it was unsealed, so as to admit air. He states as his conclusion that only a part of the air can combine with metals or be used for purposes of respiration, and that hence the air is not a simple body as generally believed, but composed of different substances; and he adds that his experiments on the calcination of mercury, and the revivification of the calx, singularly confirm him in this opinion.

At the Easter Meeting of the Academy in 1775, Lavoisier read a memoir, "Sur la nature du principe qui se combine avec les métaux pendant leur calcination et qui en augmente en poids." In a footnote we are informed that the first experiments described in the memoir were made more than a year previously, while those relating to the mercury *precipitatus per se*, "ont d'abord été tentées au verre ardent dans le mois de Novembre, 1774." Having heated calx of mercury with carbon, he found that fixed air soluble in water was given off, while when he heated it alone he observed *avec beaucoup de surprise* that an air was produced insoluble in water, readily supporting combustion, serving for the calcination of metals; incapable of precipitating lime water, and incapable of being absorbed by alkalies.

Priestley obtained a gas from mercury, *calcinatus per se*, on August 1, 1774, and finding it insoluble in water, and capable of readily supporting combustion, concluded that the mercury during calcination had absorbed *nitrous particles* from the air. He did not discover the real nature of the gas till March, 1775. In October, 1774, Priestley visited Paris, and mentioned to Lavoisier, Leroy, and others the prodction of gas from the mercury *calcinatus per se*. Probably the properties were not demonstrated. Lavoisier says he observed "with much surprise" that the gas was not absorbed by water, &c., was not in fact fixed air. He had expected to find the air given off by calx of mercury when heated alone, the same as that evolved when he tested it with charcoal, and was surprised to find it a different air. He enumerates the principal properties of the new gas as we know it. He burns it in a candle, charcoal, and phosphorus. He calls it air *eminemment respirable*, and *air pur;* and says it alone is concerned in respiration, combustion, and the calcination of metals.

Lavoisier constantly quotes Priestley and Scheele in connection with oxygen; again and again he speaks of that air which Mr. Priestley calls *dephlogisticated*, M. Scheele *empyreal*, and I *highly-respirable*," but we can find no distinct claim to its discovery save the sentence quoted above, in which he states that it was discovered *almost at this same time* by Priestley, Scheele, and himself.

In his next memoir, "On the Existence of Air in Nitrous Acid" (read April 20, 1776), he says: "Je commencerai, avant d'entrer en matière, par prévenir le public qu'une partie des expériences contenues dans ce mémoire ne m'appartiennent point en propre; peut-être même, rigoureusement parlant, n'en est-il aucune dont M. Priestley ne puisse réclamer la prèmière idée." And again: "Je terminerai ce mémoire comme je l'ai commencé, en rendant hommage à M. Priestley de la plus grande partie de ce qu'il peut contenir d'interessant." Moreover, in giving an account of ammonia, sulphurous acid, and several other gases, he writes: "Les expériences dont je vais rendre compte appartiennent presque toutes au docteur Priestley; je n'ai d'autre mérite que de les avoir répétées avec soin, et surtout de les avoir rangées dans un ordre propre à presenter des consequences." Thus it must be admitted that Lavoisier was always ready to acknowledge the merits of Priestley.

Even supposing that Priestley had demonstrated the production of oxygen to Lavoisier before he had himself obtained it, which, however, does not appear probable, Lavoisier investigated its chief properties before Priestley knew any more of it, than it was a gas containing nitrous particles. "Till this first of March, 1775," writes Priestley, "I had so little suspicion of the air from *mercurius calcinatus* being wholesome, that I had not even thought of applying to it the test of nitrous air." Again, in speaking of an experiment made on March 8, 1775, he says: "By this I was confirmed in my conclusion that the air extracted from *mercurius calcinatus*, &c., was *at least as good* as common air; but I did not certainly conclude that it was any *better*." At this time Lavoisier had proved the principal properties of the new gas, as we now know them. No wonder he expresses surprise. Did Paracelsus discover hydrogen? or did Boyle? or Mayow? or Cavendish? Lavoisier saw *with much surprise*, not that a gas was produced by heating calx of mercury, but that the gas was different from fixed air.

Let us finally examine Dr. Thomson's criticism of the "Opuscules Physiques et Chimiques":—

"Nothing in these essays," he writes, "indicates the smallest suspicion that air was a mixture of two distinct fluids, and that only one of them was concerned in combustion and calcination; although this had been already deduced by Scheele from his own experiments, and though Priestley had already discovered the existence and peculiar properties of oxygen gas. It is obvious, however, that Lavoisier was on the way to make these discoveries, and had neither Scheele nor Priestley been fortunate enough to hit upon oxygen gas, it is exceedingly likely that he would himself have been able to have made that discovery."

Now these essays were published "*au commencement de* 1774," at which time we have abundant evidence from other memoirs that Lavoisier *had* more than suspicion "that air was a mixture of two distinct fluids, and that only one of them was concerned in combination and calcination." Moreover, this had *not* "been already deduced by Scheele from his own experiments; neither had Priestley "already discovered the existence and peculiar properties of oxygen gas."

We do not the least press the following point. We trust we have made out our case without the necessity of resorting to it; but we venture to ask upon what authority Dr. Thomson asserts that "Dr. Priestley informs us that he prepared this gas in M. Lavoisier's house in Paris, and showed him the method of procuring it in the year 1774." In our edition of Priestley's works (3 vols. 8vo. "Being the former six volumes abridged and methodised with many additions." Birmingham: Thomas Pearson, 1790), Priestley, after telling us that he visited Paris in October, 1774, says, "I frequently mentioned my surprise at the kind of air which I had got from this preparation to M. Lavoisier, Mr. Le Roy, and several other philosophers, who honoured me with their notice in that city" (p. 109). And again, "as I never make the least secret of anything I observe, I mentioned this experiment also, as well as those with the *mercurius calcinatus*, and the red precipitate to all my philosophical acquaintances at Paris and elsewhere; having no idea at that time, to what these remarkable facts would lead." It is of course a very different thing to mention an experiment to an acquaintance, and to actually perform it before him. But suppose, as Dr. Thomson asserts, that Priestley had prepared the gas from *mercurius calcinatus* in Lavoisier's house in October 1774, it is abundantly manifest by his own confession that he had no idea at that time of the nature of the gas; and more than five months afterwards that he had "so little suspicion of the air from *mercurius calcinatus* being wholesome, that I had not even thought of applying to it the test of nitrous gas"; and even so late as March 8, 1775, he did not conclude that the new gas was any better than common air!

Who is the discoverer? Is it the man who obtains a new body for the first time without recognising that it is different from anything else, or is it the man who demonstrates its true nature and properties? If the former Eck de Sulzbach discovered oxygen in 1489, and Boyle in 1672 not only procured hydrogen but proved its inflammability. If the latter, assuredly Lavoisier discovered oxygen.

But whatever the verdict may be, the memory of Lavoisier shall be saved from any imputation of unfairness. He was the most generous of men. His noble character stands out clearly and luminously in all his actions. He was incapable of any meanness.

We cannot for one moment compare the work of Priestley with that of Lavoisier. The elegant methods and admirable diction of the latter contrast strangely with the clumsy manipulation and prosy phlogistianism of the former. "From an ounce of red lead," writes Priestley, "heated in a gun-barrel, I got about an ounce measure of air, which altogether was worse than common air, an effect which I attribute in great measure to phlogiston discharged from the iron. The production of air in this case was very slow." Then he heated, without method or reason, as Hales had done before him, "flowers of zinc, chalk, quicklime, slacked lime, tobacco-pipe clay, flint, and muscovy talck, with other similar substances, which will be found to comprise almost all the kinds of earth that are essentially distinct from each other, according to their chemical properties," in the hope of getting some phlogisticated air from them. What a farrago! John Mayow, a century earlier, wrote more scientifically: "Si ad flammæ naturam serio attendamus, et nobiscum cogitemus, qualem demum mutationem particulæ igneæ subeunt, dum eædem accenduntur: nihil aliud certe concipere possumus, quam particularum ignearum accensionem in motu earum pernicissimo consistere. Quidni ergo arbitremur, particulas salinas ad ignem constandum præcipue idoneas esse? Quæ cum maxime solidæ, subtiles, agilesque sint, motui velocissimo, igneoque obeundo multo aptiores esse videntur, quam particulæ sulphureæ, crassiores mollissimæque."

Priestley's observations read like the writings of the seventeenth century, Lavoisier's like those of the nineteenth. Compare with the extract given above about the "phlogiston discharged from the iron" the following, "I have," writes Lavoisier, "a salt of unknown composition: I put a known weight in a retort, add vitriolic acid and distil. I obtain acid of nitre in the receiver, and find vitriolated tartar in the retort, and I conclude that the substance was nitre. I am obliged in this reasoning to suppose that the weight of the bodies employed was the same after the operation as before, and that the operation has only effected a change." "J'ai donc fait mentalement une équation dans laquelle les matières existantes avant l'opération formaient le premier membre, et celles obtenues après l'opération formaient le second, et c'est réellement par la résolution de cette équation que je suis parvenu au résultat. Ainsi, dans l'exemple cité, l'acide du sel que je me proposais d'examiner était une inconnue, et je pouvais appeler x. Sa base m'était egalement inconnue, et je pourvais l'appeler y; et puisque la quantité de matière a du être la même avant et après l'opération, j'ai pu dire $x + y +$ acide vitriolique $=$ acide nitreux $+$ tartre vitriolé $=$ acide nitreux $+$ acide vitriolique $+$ alcali fixe; d'où je conclus que $x =$ acide nitreux, $y =$ acide fixe, et que le sel en question est du nitre."

There is nothing in Priestley's scientific writings which exhibits so masterly a treatment as this. Priestley ignored Lavoisier's brilliant conclusions. He died defending the theory of Phlogiston. He denied the decomposition of water. He worked without method or order; and without the balance; and reasoned upon facts which lacked verification by quantitative means.

His conclusions were frequently hasty and ill founded. Lavoisier's work requires no praise in this place. Priestley's discoveries may be compared to the mingled chaos of ὁμοιομερείαι of Anaxagoras; Lavoisier was the Νοῦς, the designing intelligence which set them in order, and put each in its appointed place. Not without reason, said M. Wurtz, "La Chimie est une science française. Elle fut instituée par La voisier d'immortelle mémoire."

G. F. RODWELL

A NEW DREDGING IMPLEMENT

HAVING recently visited Oban, in company with a friend for the express purpose of obtaining living specimens of Pennatulida, and of testing the powers of an instrument devised for their capture, I send you a note of our experiences which may perhaps be of interest to your readers.

The ordinary dredge, though well adapted to obtaining most animals that dwell on the sea-bottom, will clearly not do for all, and for no animal form is it less suited than for the one we were most anxious to obtain—*Funiculina quadrangularis*. This giant Pennatulid consists of a tall fleshy rod-like axis, three to five feet or more in length, and about half an inch in diameter, which bears along its sides the individual polypes of the colony, and is traversed throughout its entire length by a flexible calcified stem. *Funiculina* lives erect, with the lowermost six or eight inches planted as a stalk in the mud of the sea-bottom, and the major portion of its length projecting up freely into the water.

For such a form the dredge is clearly very unsuitable. Indeed unless the dredge be of very great size it must be a pure accident if specimens ever get into it at all. The tangles give a better chance, and yet for such a purpose they are but a clumsy and haphazard contrivance; and even should they by chance entangle and draw out a *Funiculina* there is a danger, amounting almost to certainty that it will drop off again during the process of hauling in.

The instrument we employed was a modification of one originally devised by Dr. Malm of Göteborg, and used by him with considerable success in dredging for *Funiculina* in Gullmarn Fiord, Bohuslän. Dr. Malm's apparatus, of which he has kindly furnished us with a description and drawings, consisted of three poles, each nine feet long, connected together at their ends, so as to form a triangle; the poles were armed with large-sized fish-hooks, and the dredging-rope attached at one angle, the whole apparatus strongly resembling that used by the Philippine Islanders for dredging *Euplectella*, as described and figured by Moseley (Naturalist on the *Challenger*, p. 407).

Our instrument, as we first used it, consisted of two poles six feet long, connected together in the form of a letter A by a cross-bar four feet long. The rope was fastened to the apex of the A, and lead weights to the lower ends of the side poles. Attached along the cross-bar at intervals of six inches were cords four feet in length, each armed with five or six fish-hooks and having a small lead weight tied to its lower end. The theory of the machine was that the whole instrument would be dragged along at an angle of about 30° to the sea-bottom, steadied by the weights at the ends of the side poles; the cross-bar being a foot or so above the ground, and the cords armed with fish-hooks trailing behind, with their ends kept on the bottom by the small weights attached to them.

The machine was subsequently modified by lengthening the cross-bar to nine feet, and attaching the fish-hooks not singly, but in threes, like grappling irons. We also connected the cords together by horizontal strings, in order to obviate their tendency to become entangled with one another.

The instrument yielded excellent results: a large number of specimens of *Funiculina quadrangularis* were obtained, four or five, and in one case as many as seven being brought up at a single haul; the specimens were also in perfect condition, the injury inflicted by the hook being quite imperceptible. Several of the specimens were of large size; and one dredged in Ardmucknish Bay, and measuring no less than sixty-five inches in length, appears to be the largest specimen hitherto obtained alive from any locality, being a foot longer than the largest recorded by Kölliker in his monograph on the the Pennatulida. Even this, however, does not appear to be the limit of growth, for a dead stem obtained at Glaesvae, in the Bergen Fiord, and now in the Hamburg Museum, is more than seven feet in length.

Funiculina quadrangularis is generally considered a rare species. It is certainly a very local one; but our Oban experience would lead us to infer that where it does occur it is to be found in quantity, an inference borne out by Sir Wyville Thomson, who speaks of passing over a "forest of *Funiculina*" when dredging in Raasay Sound during the *Porcupine* expedition. It appears to have been hitherto obtained at Oban only in small numbers, a result we believe to be due entirely to the use of instruments ill-adapted to its capture.

Four or five specimens of *Pennatula phosphorea* were obtained with the same instrument, which further proved its utility by bringing up several fine specimens of Hydrozoa. The instrument in its present form is clearly capable of improvement; still the results of a first trial have been so good, that we may possibly be rendering a service to other naturalists by making them known through your columns.

A. MILNES MARSHALL

Owens College, October 27

WIRE GUNS

IT will no doubt surprise many of our readers to be told that after nearly a quarter of a century of experiment and investigation, and the expenditure of millions upon millions of money, the nation is so imperfectly armed that we are again entering upon a period of reconstruction of our heavy ordnance, the outcome of which it is not easy to foresee. From the old cast-iron 68 pounder, weighing from 4 to 5 tons, we have arrived at the 80 ton gun of Woolwich, but only to learn that such guns are already obsolete, and must give place to others of a new type developing greater power with less weight. Till very recently we have been constantly told by the highest authorities in this department of the Government that the English guns were the finest, the strongest, and the most powerful in the world, and it is no doubt somewhat startling to learn that all this has been a delusion.

It is not our intention to dwell upon the causes of this, nor to inquire whether it has been due to departmental conservatism or to the uncertainty incidental to the progress of an art carried on by a tentative method, and modified from time to time by new discoveries in physical science. Our purpose is rather to give some information about a system of gun making, which is at last obtaining the attention of gunmakers, we allude to what is termed the wire system of construction.

Twenty-seven years ago this system was brought before the then existing Ordnance Committee by the writer who has from that time to this persistently advocated its merits, proving, not only by the construction of guns but also by mathematical analysis, its great advantage over other systems; but it is only within the last two or three years that it has been regarded with tolerance by practical gun makers.

In France the system has been applied under the superintendence of Capt. Schultz, of the École Polytechnique, and in this country Sir Wm. Armstrong and Co. have made one or two guns, the latest and largest of